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1539 Brannton St.  
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 55109. Mar 19, 1984

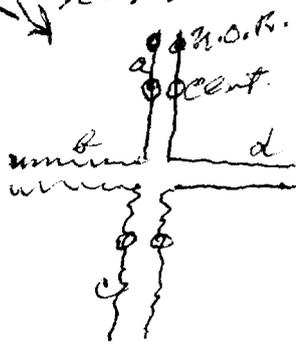
Dear Barb,

When you find time, tell me about your trip to receive the Nobel Prize, events there, etc. you were competing in the press with the Peace Prize given to Nolinga (his wife to receive it). I have a grad student living here, a Ph.D. student in Soils (Peat studies). His mother sent a clipping from the local paper in Mankato - it had a short account on your talk.

The past few days I have been reviewing a paper by Kodera, <sup>sumi</sup> Andhra, Walter, India. - on segregation from chain configurations in three translocations in Pearl Millet, one of which always forms chains. He has an earlier paper in Genetic Res. 34: 69-86 1979 which I must read. (Metaphase - 1 observation, spore quartets)

at any rate, he has convincing evidence that the kinds of segregation depend on which arm of the pachytene cross fails to remain associated (or fails to pair).

pairing always fails in arm a or nothing about behavior of chains



In the O4 configuration, the four kinds of segregation occur (may occur) in meiosis that don't cross over in interstitial segments: alternate-1, -2, adj-1, -2. If failure to be paired occurs in arm b or d, only alternate-1 and adjacent-1 occur. If pairing fails in arms a or c (the centric, non-translocation arms, only alternate-2 and adjacent-2 occur.

The ones I have studied in corn are all of the type with failure in arms b or d to form the chains. Phillips and I will try to select the other type for work this summer, what follows are some personal recollections.

Sincerely Charles B.

1

These are memories that involve the two of us, I hope you don't mind. Mar. 19, 1984

Do you remember when I first met you in the cornfield - was it on a Sunday? Seed I had sent had been planted, and I went to the field to see it. Together we located the plants and figured out the planting arrangement. \* 55 years ago this June.

I had read your note in Science on the corn chromosomes. I had taken all the courses in botany except taxonomy and the lab in Cytology (Allen). One of the courses with C. E. Wilen was "Facts and Theories of Heredity", in addition to his lectures on cytology.

My material obviously needed some cytology. You set up another small table at the end of a partition (wall divider?) - almost opposite where you were working. I was struggling to make slides in which I could see the chromosomes. You were continually showing me what you were getting - chromosomes clearly showing what you said was there. Finally I did get slides in which I could count the chromosomes <sup>(4n)</sup> and could do it repeatedly. I don't remember, but you must have followed your instructions and ~~took~~ <sup>took</sup> sperocyte samples when tassels began to shed pollen. I began classifying the plants for sterility and also for  $1x-2x$  - you found how to get good classification for  $1x-2x$  pollen with the  $I_2-KI$  stain - cut off the light from below the microscope stage. I had stumbled onto it, but had not known what I had done to get the good classification. I tabulated the results on a movable blackboard in the room & it was soon evident that in one group there was a ratio of  $1N:2$  semisterile: 1 high sterile with a few having a intermediate amount of pollen abortion, when I looked at sperocytes from the high-steriles, there were 2 @ 4. Also there was a linkage between  $2x$  and sterility, Brink and I were using a  $pr-2x$  stock as a standard normal - also that stock had the large terminal knob on 9S - what luck!

from my material.

Mar. 18, 1959.

Tabulations from another group showed nearly all plants were semisterile, with a few having low sterility. After I found the 2 O4 for the other group, I began to try to find an explanation for the other result. One night it suddenly dawned on me that if one chromosome were involved in the two, there would be a O6 in the high steriles (3H). I went to the lab early the next morning and believe had a slide showing it when others came in. That is the way I remember it - do you remember that? It was a great thrill for me.

You were getting results from the trisomics, cytology and genetic segregations. Others were getting results: linkages, etc., sharing information and pollen from stocks to make crosses needed to make further tests, etc. Beadle was doing cytology on male steriles. For one of them, it was quite a while before I realized that one that he was talking about was not A-synaptic, something to do with the H Alewone factor, but a leach of synapsis. As I was working at my microscope, it suddenly hit me - and I blurted out the explanation - realization as to what it was. You commented in such a way, that I realized ~~another~~ had drawn conclusions <sup>about me</sup> from my previous confusion. Personal! Do you remember this occasion? As I worked along at my microscope and you continued to show me what you were finding, on one occasion I was ~~so~~ overcome with emotion - obvious to you, and your response was "oh!" Nothing more was said at the time. That night I did a lot of thinking and decided to "cool it" - I didn't want to jeopardize our friendship. Maybe that's why we have been able to ~~continue~~ continue our easy,

relaxed friendship? 3.

Do you remember when, <sup>during corn pollination</sup> I had one of my annual stomach upsets and went over to a tree on the north side of the field and laid down under the shade? You came over to talk with me.

I think we shared ideas and information all summer. Then I went to the Bussey Institution for the winter, back to Cornell for the next summer, then to Cal Tech. Was that the summer Marcus came back from Cal Tech? Just before I left on my trip, I went with Harriet on a short farewell drive. When I returned, you asked her "Did you tell him?" She replied "No". Then you and I went for a short drive. I was so excited about going on the trip and to Cal Tech, very little else registered. Also, the job situation was such that I could offer no security. Attitudes then were different from what they became later.

Those were exciting times: linkage groups <sup>being</sup> associated with chromosomes - your work, pachytene analysis of my 8-9a (your work), Harriet's and your paper on cytological proof that genetic crossovers were correlated with cytological exchanges. It was one of my stocks that made part of the results not as clearcut as they might have been. I had had trouble getting plants from cah seeds, and had ~~supplemented~~ supplemented them with cah seeds - plants from these latter seeds were one source of pollen.

Then you came to Cal Tech while I was there. I was working with a loco-sterile stock, showed you some slides. You came back the next day with the analysis. I was a little "taken aback" at first, you noticed it from what you told Andy - but my reaction soon passed off, as I realized the intensity of your interest and ability to interpret what you saw under the microscope.

When I was asked about teaching a course in cytological ~~the~~ technique, you made suggestions on what to do. In that class, during the period when the cah near technique was the exercise, every student was  
\* I still have your sketches

able to get a slide in which he had cells good enough to get the chromosome count. The only time it ever happened for me.

The Morgans (H. and L.V.) invited us to their home. Another episode: One of the men invited me to a party - when I arrived, he asked about my wife - he had thought he was inviting Beadie. I did not respond as I should have to ease his feelings - I never did develop any of the social graces - as you well know.

Then I went to Missouri - some work on pachytene of *Sorghum versicolor* - only 5 chromosome pairs, but I never did get a spread where all the chromosomes could be followed. I tried taking spermatocytes at different times during the day, and at night - no luck. You came there - was it to work on the deficiencies for various gene markers - result: the Mo. Bulletin? I don't remember what I was doing on corn - it might have been the pollen abortion transmission studies, along with more on interchanges. I remember Stadler saying that these probably was a simple trick to building the big ring - I came close, but it remained for a student of mine, Dr. Man, to find the trick - the basis for his thesis and what I describe in my book as the Dr. Man scheme: crosses between parents that have the same interchange or interchanges in common but involving in each <sup>parent</sup> an additional chromosome. Each step produces 2 @ 4, no sterility higher than that. I have tried to omit some intermediate steps, but crossovers that break down the rings give problems. Dr. Man's scheme avoids that but for 100, it is a long process. I am still working on that. It ends up with one stock with all the interchanges necessary, so that it  $\times N = a @ 20$ . I remember one evening you, Stadler, and I were coming back from some event - I left (was it on a bus) - and the next day you asked why I had left - my response was "there is a crowd".

Sometime that fall I went back to Cornell. Emerson gave me the job of "calculating and checking the recombination values for their linkage summary". Also, I helped several Chinese students with their theses. I believe that might have been when Emerson, Beadie, and I went to visit Eyster (Bucknell Univ.) - He showed us his work, a tremendous amount of linkage data with new traits on cards. He promised to furnish stocks

I don't remember if he gave me his stocks then or sent them to Emerson later. We visited him because he usually failed to send stocks. He had sent his Crown Midrib stock to me in response to my request. I had happened to make a cross that involved pr and Cr - my first finding of a linkage - exciting for me.

Dr. Reddick at Cornell had a problem that involved crosses between *Bolanum desmianum* and *stuberosum*. I had applied for a Sterling fellowship at Yale Univ. but also applied for work with Reddick. Both were awarded, but I went to Yale. While there a job at West Va. opened, and I obtained that job - it involved teaching an advanced course in genetics and cytogenetics, breeding work with corn, field corn breeding and Stewart's wilt disease resistance in sweet corn; watermelon wilt disease (crosses with the Russian wilt resistant species), and winter barley. I could also continue my corn cytogenetics. There I met a woman from Mo., and we were married. A job at Davis, Calif. opened up. I was offered the job, accepted, but after a few weeks, I received a telegram that the appointment was not approved - no explanation, ~~and~~ I had not resigned at West Va., continued there until 1978, when I came to Mo. Min.

I believe you came to Mo. Min. once for a visit and called on my wife. She didn't say anything specific, but I had the impression that you wanted to assure yourself that my situation was O. K. My wife was good at understanding people and getting their cooperation. As I look back on my married life, I realize my wife was very tolerant - she must have been to put up with some of the things I did.

There was never any interference with my work, and she was free to participate in whatever activities she wished: League of Women Voters, Faculty women's Club etc., also various activities for our daughters.

Oh, yes, when I was at Mo., Stadler was planning to attend the Genetics Congress at Othaca. I planned to drive - Luther Smith and another student plus Stadler. Stadler had to have more time to finish his paper - we started late and arrived at Othaca about noon on the opening day, after driving all night.

Where were the meetings at which you and I had material at a poster session? I believe I had tabulates the number of breakpoints per chromosome based on the interchanges we had. I remember K. A. Fisher commented that the numbers fitted a Poisson distribution. You

pushed me to talk to people as they came in.

We were both at the Xmas meetings, "BAGS" in New York. I. Ibrahim, one of my students was with me. You hadn't had a chance to talk to me about your work with the AC-DS system, but came with us to the train station and went through the whole story. When we left on the train, my student and I immediately wrote what we remembered from what you told us. There were some questions, so I wrote to you and you answered them. I included that in the mimeographed material that I used as hand-outs in my advanced Genetics and Cytogenetics courses here.

When <sup>from</sup> Phillips was at Cornell on a Post-Doc Fellowship, I was invited there to give some seminars. Again, we had chances to talk - then your visit to Minn.; seminars, sessions with grad students, discussion of ears that Ron had showing AC-DS effects - your comments were taped.

Then I attended the Stadler symposium at Mo, the one at which you gave a talk - very warm greetings again - I attended their evening banquet - we should have spent it talking, since you were not planning to attend - I had not realized you were not planning to attend.

The next occasion was the Maize Breeding, Illinois meeting at which we had several opportunities and interesting exchanges. I remember you told me about all the party finery - clothes, shoes, etc. you were discarding - signals?

Our talks at the July, 1982 celebration of the 75th Anniversary of the Synapsis ~~Club~~ at Ithaca, again, were warm and friendly. I know that they were <sup>and are</sup> mutual. I would like very much to have the chance, again, to talk. Is there any possibility that you would be interested in spending a little time here this summer? Phillips and his group are doing some very exciting things, also Rubenstein, Messing and their group. I believe Messing talked with you recently.

We obtained second backcross <sup>to American</sup> of Chinese x American chestnuts last summer (300) I say we but others made the crosses, and hopes are to get more this year. I have appreciated all the support you have given me over the years.

Sincerely, Charles B.